

Microbiology

N. Y. U.

477, 7th Avenue

New York 16, N.Y.

7 Jan '52

Dear Tom,
Thank you for your letter, and New
Year wishes, also for your Christmas card, which
with others now adorns the wall of my apartment.
I am most grateful to you for your most
flattering invitation to spend a year at Wisconsin,
but, very much to my regret, I do not think I
shall be able to take advantage of it: I should
probably have been able to get my fellowship
from the Commonwealth Fund extended, but
the difficulty is my teaching commitments in
London: I get leave more or less on the
understanding that I should be back for the
beginning of next academic year, and I know well
that because of other staff movements, my department
would be in some difficulty in getting my work done
if I did not turn up. (I know this sounds like the delaying

of indispensability on my part.) Thank you also for your generous offer of a temporary appointment.

I am, of course, most anxious to see what I can of your work; I gather from Norton (who I only saw for $\frac{1}{2}$ hour or so, owing to his having been unwell, & I having to go off elsewhere shortly after he looked in) that your lab. work is more or less full blast through the summer: my fellowship normally would expire in June, but I am ~~as~~ fairly sure of getting a three months extension. If you think it a good idea, I should certainly like to spend those three months at Wisconsin: I ~~had~~ had originally intended to spend the summer travelling round the country (I get a special allowance for travelling, and in fact most years 2 months travelling as a condition of my fellowship.) I now realize that I am not going to get anything very great accomplished in the Pneumococcus transformation field in the time I have available, & that it might be better to go off round the country earlier, & spend some time with you, if that is feasible. I hope you will let me know what you think about this, I know that

3 months is not long enough to get anything much done
but it would at least enable to pick up your techniques,
& perhaps get started on something, to finish later.

I have not got any results yet on pneumo. transforming
principle filtration; I have been using my time on trying
to find other characters, i.e. growth requirements, which
might be induced to give more quantitative data, i.e.
something for which one can screen, so far without any
results: even on the fairly complex semi-defined medium
they use here it is difficult to get satisfactory growth on agar,
and the cocci ^{have} ~~leaving~~ a suspending habit^s! during a
autolyzing if conditions are not such as Ranta Nam:
apparently no one has isolated any pneumo. phages, so
there seems nothing left except antibiotic resistance.
- Hostobris tells me that even a penicillin resistance the
quantitative aspects are full of doubts as to interpretation.
I hate not being able to titrate a thing to even a $\times 2$ or $\times 2^2$
accuracy, especially as it more or less rules out
experiments on adsorption & saturation of cells, comparable
to your - Norton's or S. ty-mur. factor.

A propos flagella, I have no personal knowledge
of the phenol suppression: I have been told, however,
by people who run labs. making vaccines a suspension

for agglutination tests that it is not uncommon to get a batch of nutrient broth which is satisfactory for growth but gives very poor development of H antigen, possibly this is comparable with the (reported, I have not tested it) failure of flagella to develop on a synthetic medium. If the latter phenomenon occurs, it may perhaps be analogous to the low titer of ^{nitratase} ~~nitrate~~
"non-essential" ^{nitrate} ~~nitrate~~ e
Other constitutive enzymes of cells grown on synthetic agar medium.

In Racker's old ^{old} work he did some interesting (unpublished) work on swarming in Pasteur which might be relevant. On synthetic agar + nicot. acid there was good growth but no swarming, the most active constituent of complex media in restoring swarming was found to be glutamine (or its precursors).

As to the H-specific phages, Mark Adams tells me Rakieten is no longer at Brooklyn, & he does not know where he is. However, having recently heard from Aszkenasy, of Bronx Botanic Garden, that Bilgakov, (of the lab. ~~that~~ du bacteriophage 75 Rue Olivier-de-Serres, Paris XV) who was I think co-author with Sertie, was still alive & active, I wrote to him about it, & got a reply a couple of days ago in which he says he thinks he

will be able to send me the phage as soon as we
has resuscitated it. I will send it you when I get it.
I have always thought it a most extraordinary thing. I
shall be interested to see whether it selects stable O's,
or only (or mainly) unstable poorly "flagellated" variants.
I thought at one time that they might provide
one side of a system in which one could measure
mutation rates in each direction with an
appropriate screening medium for each.

I was most interested, & just fascinated, to hear
of the experiments on transferring *typhi-murium*
flagella to *S.typhi*. I am still brooding on the
significance of the non-mutability of phase of the
transformed type. If rate of mutation is
controlled by a second single gene, *ctb^{typhi}*,
S.typhi murium transformed to H by means of ex-
filtrate of *S.typhi* (if this is possible) should
readily mutate, to presumably be second "artificial"
phase of *S.typhi*.

I don't think I have anything else to say.
Thank you again for your generous invitation.

Best wishes for 1952 to you and Esther

Yrs sincerely

Bruce Storer